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PROFESSOR AGRAMONTE AND THE ETIOLOGY OF YELLOW FEVER.

BY

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PROFESSOR AGRAMONTE AND THE ETIOLOGY OF YELLOW FEVER.

To the Editor of the Medical Record:

Sir:—In the Medical Record for March 30, 1912, Professor Agramonte has attacked me and my recent paper on the etiology of yellow fever ("Yellow Fever Bureau Bulletin," November, 1911) in terms which ought not to enter in a scientific discussion, and which I therefore prefer to leave unanswered. With regard to the question, whether I have discussed in a fair manner or otherwise the work of the U. S. Army Commission, I would simply ask readers to compare the statements in my paper with those of Agramonte. Leaving aside non-essential discussions, two points remain to be considered.

Agramonte is offended because I have said that the experiments of Reed, Carroll, and Agramonte are not absolutely conclusive, not having been carried out in a non-infected country, as were the corresponding experiments in malaria and pappataci fever. In this opinion I do not stand alone, as Agramonte well might know. The question cannot be resolved by an appeal to authorities, nor can it be modified by the objections which can be raised against a repetition of the experiments. Agramonte says that the danger of such experiments should restrain experimentation within certain limits, a quite unnecessary remark as directed against me, since I

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already said just the same in my paper which Agramonte criticises. But if moral objections prevent conclusive proof from being obtained, relative proofs do not become as a consequence conclusive.

The remark that the prophylactic results "stare the least observing in the face" is of a similar nature, namely entirely superfluous and misleading as giving the impression that I had not emphasized this important fact in my paper. The truth, however, is that I mentioned the results obtained by antimosquito measures as constituting the most important corroboration which the theory has received.

Next, as to the bodies which I have described as blood parasites in yellow fever cases and therefore presumably the pathogenic germ in this disease. The question of their existence cannot be resolved by Agramonte nor by anybody else on the strength of *a priori* reasoning as to the period during which the parasites ought to be present in the blood, or the like. It is simply a question of observation, and here we arrive to the only real point in Agramonte's criticism. He did not feel convinced by examining the specimens which I showed him; this opinion I must respect, though still preserving the hope that he may change his opinion on some future occasion. I must, however, at the same time claim respect for my statements also. Agramonte may differ in his opinion as to the significance of the bodies demonstrated, but he has seen these bodies and he has no right whatever to throw doubt on my statements about the numbers of cases in which similar bodies have been observed.

When mentioning the demonstration in Havana, it does not seem fair to convey the idea that the impression which Agramonte, according to his paper, received was that of all gentlemen present.

Agramonte might have added that at least one distinguished colleague, who had been present during the whole time of the demonstration and had seen all the specimens demonstrated and not four only, as Agramonte declares that he saw, gave as hsi opinion that the bodies presented were decidedly parasite-looking, but with regard to their importance suggested the possibility that one might have to do with innocent parasites living in the blood of the inhabitants of Yucatán. I may possibly be wrong in my interpretation of my observations, but as to the existence of the bodies there can be no doubt, and during my present investigations in Yucatán I have found them again and again.

HARALD SEIDELIN, M.D.

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